Logical nihilism

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> "This seems to abolish logic, but does not do so."
> —L. Wittgenstein, *Philosophical Investigations* §242

1. Introduction

The idea that there may be more than one correct logic has recently attracted considerable interest. This cannot be explained by the mere fact that several distinct logical systems have their scientific uses, for no one denies that the "logic" of classical mathematics differs from the "logics" of rational decision, of resource conscious database theory, and of effective problem solving. Those known as "logical monists" maintain that the panoply of logical systems applicable in their various domains says nothing against their basic tenet that a single relation of logical consequence is either violated by or manifest in each such system. "Logical pluralists" do not counter this by pointing again at the numerous logical systems, for they agree that for all their interest many of these indeed fail to trace any relation of logical consequence. They claim, instead, that no one logical consequence relation is privileged over all others, that several such relations abound.

Interesting as this debate may be, I intend to draw into question the point on which monists and pluralists appear to agree and on which their entire discussion pivots: the idea that one thing a logical investigation might do is adhere to a relation of consequence that is "out there in the world," legislating norms of rational inference, or persisting some other wise independently of our logical investigations themselves. My opinion is that fixing our sights on such a relation saddles logic with a burden that it cannot comfortably bear, and that logic, in the vigor and profundity that it displays nowadays, does and ought to command our interest precisely because of its disregard for norms of correctness.

I shall not argue for the thesis that there are no correct logics. Although I do find attempts from our history to paint a convincing picture of a relation of logical consequence that attains among propositions (or sentences, or whatever) dubious, I should not know how to cast general doubt on the very idea of such a relation. By "drawing this point into question" I mean only to invite reflection about what work the notion of a correct logic is supposed to be doing, why the debate about the number of logical consequence relations is supposed to matter to a logician, and whether the actual details of logic as it has developed might be difficult to appreciate if our attention is overburdened by questions about the correctness of logical principles. Rather than issue any argumentative blows, I propose merely to lead the reader around a bit until his or her taste for a correct logic sours.

2. The law of excluded middle

Surely the most notorious bone of contention in the discussion of logical correctness is the law of excluded middle, $\phi \lor \neg\phi$. Is this law logically valid, so that we know that its instances, "Shakespeare either wrote all those plays or he didn't," "Either the continuum hypothesis is true or it is not true," "He's either bald or he isn't," etc., each are true in advance of any further information about the world?

One hardly needs to mention that hundreds of spirited disavowals and defenses of *lem* have been issued in the last century. Many of these have even been authored by expert logicians. But let us turn our backs to these ideological matters and consider briefly some of what we have learned about *lem* quite independently of any question about its correctness.

The simplest setting for this is the propositional calculus.[1] The intuitionistic propositional calculus (IPC) differs from the classical propositional calculus (CPC) precisely in its rejection of *lem*. For a concrete and standard formalization of IPC one may take a typical Hilbert-style axiomatization of CPC and erase the single axiom for double negation elimination, $\vdash \neg\neg\phi \supset \phi$, leaving the rules of inference as before.

In fact, one of the first things observed about *lem* is its equivalence with *dne*, which is most easily seen in the setting of natural deduction. Standard natural deduction presentations of CPC have a rule allowing one to infer any formula ϕ from the single premise $\neg\neg\phi$. It is easy to derive in such a system the formula $\phi \lor \neg\phi$. It is similarly easy to show that if we modify the calculus by disallowing *dne* but now introduce a new rule that allows one to write down any instance of *lem* in any context, ϕ will be derivable in all contexts in which $\neg\neg\phi$ is derivable.

In *1934–35*, Gentzen observed to his own surprise[2] that the sequent calculus presentation of CPC admits an even more elegant modification into a presentation of IPC than the one just described. One simply disallows multiple-clause succedents and leaves the calculus otherwise unchanged. Thus *lem* and with it the entire distinction between intuitionistic and classical logic is subsumed into the background structure of the logical calculus. All the inference rules governing the logical particles $(\land, \lor, \supset, \neg)$ and all the explicit rules of structural reasoning (identity, cut, weakening, exchange, and contraction) are invariant under this transformation. Thus it appears that a duly chosen logical calculus allows a precise analysis of what had been thought of as a radical disagreement about the nature of logic. When classicists and intuitionists are seen to admit precisely the same inference rules, their disagreement appears in some ways quite minor, if more global than first suspected.

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[1] I emphasize that this really is a matter of perspicuity. One should not think that the phenomena described below are artifacts of peculiar features of propositional logic. They are nearly all consequences of decisions about *lem* that are invariant across a wide spectrum of logics. Consider: the structural subsumption of *lem* applies also to the predicate calculus; the admissible propositional rules of Heyting Arithmetic (and of Heyting Arithmetic with Markov's principle) are exactly those of IPC (*Visser 1999*); the disjunction property holds for Heyting Arithmetic (*Kleene 1945*) and for intuitionistic Zermelo–Fraenkel set theory (*Myhill 1973*).

[2] Gentzen described the fact that *lem* prescribes uses of logical particles other than those given by their introduction and elimination rules as "troublesome." The way that in the sequent calculus the logical rules are quarantined from the distinction between classical and intuitionistic logic he called "seemingly magical." He wrote, "I myself was completely surprised by this property ...when first formulating that calculus" (*Gentzen 1938*, 259.)

Exactly how minor, on closer inspection, is the difference between these superficially similar calculi CPC and IPC? Not very. Even before Gentzen's profound analysis, Gödel *1932* observed that IPC satisfied the "disjunction property": formulas such as $\phi \vee \psi$ are provable only if either ϕ or ψ is as well. At first sight this might appear to be no more than a restatement of the intuitionist's rejection of *lem*. After all, that rejection was motivated by the idea that instances of *lem* are unwarranted when neither of their disjuncts can be independently established. But, one might think, if any disjunction is warranted in the absence of independent verification of one of its disjuncts, those like $\phi \vee \neg\phi$ are, so rejecting *lem* should lead to something like the disjunction property. This reasoning strikes me as worthy of further elaboration and attention, but it should be unconvincing as it stands. For one thing, the formal rejection of *lem* only bars one from helping oneself to its instances whenever one wishes. The gap between this modest restriction and the inability ever to infer any disjunction of the form $\phi \vee \neg\phi$ at all, unless from a record of that inference one could effectively construct a proof either of ϕ or of $\neg\phi$, is a broad one. More, there are infinitely many formulas ϕ unprovable in IPC, such that IPC$+\phi$ is consistent but insufficient to prove *lem*. Examples like $\neg p \vee \neg\neg p$ may well fuel suspicions that even in the shadow of global distrust of *lem*, some disjunctions $\phi \vee \psi$ are more plausible than any instance of *lem* even in the absence of resources sufficient to derive ϕ or ψ.

Thus the disjunction property is a non-trivial consequence of the invalidity of *lem*. In fact, this situation exemplifies a recurring phenomenon in logic, wherein from the assumption of a special case of some general hypothesis, that hypothesis follows in its full generality. This often happens even when, as in the present case, the general phenomenon does not appear, even in hindsight, to be a "logical consequence" of its instance in any absolute sense. The disjunction property has further consequences of its own, however, to which we can profitably turn.

In the approach to semantics known as inferentialism, the meaning of a logical particle should be identified with the conditions under which one is justified in reasoning one's way to a statement governed by that particle. From this point of view, which is given expression already in some of Gentzen's remarks, and owing to the separation in sequent calculus of *lem* from the logical rules, the meanings of the familiar logical particles might be said not to differ in intuitionistic and classical logic.

However, the disjunction property gives rise to an alternative interpretation of the logical particles of IPC in which each theorem refers back to the notion of provability in IPC itself. For if $\phi \vee \psi$ is provable only if one of ϕ and ψ is as well, then a candidate and interesting reading of the sentence $\vdash_{\text{IPC}} \phi \vee \psi$ is "Either $\vdash_{\text{IPC}} \phi$ or $\vdash_{\text{IPC}} \psi$." Expanding on this idea, one might suggest that the provability of a conjunction "means" that each of its conjunctions is provable, that the provability of a conditional, $\phi \supset \psi$, "means" that given a proof of ϕ one can construct a proof of ψ, and that the provability of $\neg\phi$ "means" that a contradiction can be proved in the event that a proof of ϕ is produced.

As we shall see shortly, this so far informal interpretation of intuitionistic logic is riddled with ambiguities. All the same, some reflection should bring home the idea that some disambiguation of this reading is a possible way to understand the theorems of IPC. When one compares the situation with CPC, where conditional truth comes so cheap and the disjunction property fails badly, one can only conclude that the difference between these calculi is in some sense great after all, greater

even than the debate over the validity of *lem* alone first suggests. When one then recalls the earlier observation that these calculi can be presented so that their formal differences are slight and their rules identical, the observation that radical differences in meaning result from so subtle a change in form is striking.

If we are to take seriously the idea that theorems of IPC refer back to IPC provability, then some care must be taken in making this interpretation precise. For if the provability of a conditional, $\phi \supset \psi$, "means" that $\vdash_{\text{IPC}} \psi$ in the event that $\vdash_{\text{IPC}} \phi$, then one should expect $\vdash_{\text{IPC}} \phi \supset \psi$ in every situation in which the set of theorems of IPC is closed under the rule "from ϕ, infer ψ." However, the disjunction property implies that these expectations will not be met.

To see this, consider the Kreisel–Putnam rule, "From $\neg\phi \supset (\psi \vee \chi)$, infer $(\neg\phi \supset \psi) \vee (\neg\phi \supset \chi)$." The only derivations of $\neg\phi \supset (\psi \vee \chi)$ in natural deduction are proofs whose last inferences are instances of $\supset$-*elim* (modus ponens), *dne*, or $\supset$-*intro*. It is easy to see that a proof ending in $\supset$-*elim* cannot be the *only* way to prove this formula, that in fact any such proof can be normalized into a proof of the same formula whose last inference is an instance of one of the other two rules. If, further, we consider the prospects of this formula being a theorem of IPC, then *dne* is no longer a rule, and we may conclude that any proof necessarily contains a subproof of $\psi \vee \chi$ from the assumption $\neg\phi$ (to allow for $\supset$-*intro*). What might this subproof look like? Once more, *dne* is not an option, so again by insisting that the proof is normalized (so that it doesn't end needlessly and awkwardly with $\supset$-*elim*) we ensure that its last step is an instance of $\vee$-*elim*. But this means that an initial segment of this subproof is a derivation in IPC either of ψ or of χ from the assumption $\neg\phi$ (it is here that the disjunction property rears its head), and in each case it is clear how to build a proof of $(\neg\phi \supset \psi) \vee (\neg\phi \supset \chi)$. Putting this all together, we see that whenever $\neg\phi \supset (\psi \vee \chi)$ is a theorem of IPC, so too is $(\neg\phi \supset \psi) \vee (\neg\phi \supset \chi)$.

When a logical system's theorems are closed under a rule of inference, we say that the rule is "admissible" for that logic. The above argument established that the the Kreisel–Putnam rule is admissible for IPC. One might expect that the rule is also "derivable," that $\vdash_{\text{IPC}} \neg\phi \supset (\psi \vee \chi) \supset ((\neg\phi \supset \psi) \vee (\neg\phi \supset \chi))$. However, it is not (*Harrop 1960*). This situation is dis-analogous to that of classical logic, where all admissible rules are derivable so that the distinction between admissibility and derivability vanishes. In the parlance, we say that CPC is "structurally complete" but that IPC is not.

In fact much of the logical complexity of IPC can be understood as a residue of its structural incompleteness. For the space of intuitionistically valid formulas is far more easily navigated than the space of its admissible rules: although it is decidable whether or not a rule is admissible in IPC, there is no finite basis of rules that generates them all (*Rybakov 1997*).

What ought one make of the structural incompleteness of IPC? One thing that can definitely be said is that reading the expression $\phi \supset \psi$ as "There is a procedure for transforming a proof of ϕ into a proof of ψ" is problematic for both the classicist and the intuitionist, but for different reasons. This reading is wrong for the classicist, because the idea of procedurality simply does not inter into the conditions of classical validity. By contrast, procedures of proof transformation are central for the intuitionist. However, we now know that there *are* procedures for transforming a IPC-proof of ϕ into a IPC-proof of ψ in cases where $\phi \supset \psi$ is not a theorem of IPC. So at best one could say that

IPC is incomplete with respect to this semantics, and more plausibly one should say that this reading of $\vdash_{\text{IPC}} \phi \supset \psi$ is erroneous.

Thus we see a sense in which the phenomenon of structural completeness is related to a sort of semantic completeness: A structurally incomplete logic will be incomplete with respect to the most naive procedural reading of its connectives. It also happens that structural completeness bears a precise relation to the phenomenon of Post-completeness, the situation in which any addition made to the set of theorems of some logic will trivialize the logic by making all formulas in its signature provable. To state this relationship, we refer to a notion of "saturation." For a logical calculus L whose formulas form the set S, let $Sb(X)$ be the set of substitution instances of formulas in $X \subseteq S$ and let $Cn_L(X)$ be the set of formulas ϕ such that $X \vdash_L \phi$. L is saturated if for every $X \subseteq S$ $Cn_L(X) = Cn_L(Sb(X))$ for every $X \subseteq S$. By a (*1973*) theorem of Tokarz, a Post-complete calculus is structurally complete if, and only if, it is saturated.

For these and perhaps other reasons many authors have felt that the presence of non-derivable, admissible rules is a deficiency of systems like IPC. The very term "structural incompleteness" suggests that something is missing from IPC because correct inferences about provability in this logic are not represented in IPC. Rybakov (*1997*), for example, suggests that "there is a sense in which a derivation inside a [structurally incomplete] logical system corresponds to conscious reasoning [and] a derivation using [its] admissible rules corresponds to subconscious reasoning." He faults such systems for having rules that are "valid in reality" yet "invalid from the viewpoint of the deductive system itself" (10–11). Structurally complete systems, by contrast, are "self contained" in the sense that they have the "very desirable property" of being conscious of all the rules that are reliable tools for discovering their own theorems (476). It seems to me that this attitude derives from wanting to preserve the naive procedural understanding of the logical connectives. The situation ought rather, I counter, lead one to appreciate the subtlety of procedurality exhibited in intuitionism. For the logical lesson to be learned is that in the absence of *lem* the context of inference takes on a new role. Thus $\phi \supset \psi$ means that given any background of assumptions from which ϕ is provable, a proof of ϕ can be transformed into a proof of ψ under those same assumptions, and this understanding does not reduce, as it does with logics insensitive to context, to the idea that any proof of ϕ can be transformed into a proof of ψ. This irreducibility strikes me as a "very desirable property" for many purposes. I should like to know more about the conditions that lead to it.

From this point of view, it is natural to ask whether there are logics that, unlike classical logic, admit a constructive interpretation but, like classical logic, are not sensitive in this way to context. Perhaps the constructive nature of IPC derives from its context-sensitivity. Surprisingly, Jankov's logic, IPC$+\neg\phi \vee \neg\neg\phi$, appears to undermine any hope of establishing a connection between these phenomena. Consider the Medvedev lattice of degrees of solvability. The setting is Baire space ω^ω (the set of functions from ω to ω) and the problem of producing an element of a given subset of this space. By convention, such subsets are called mass problems, and their elements are called solutions. One says that one mass problem reduces to another if there is an effective procedure for transforming solutions of the second into solutions of the first. If one defines the lattice $\mathfrak{M}$ of degrees of reducibility of mass problems, it happens that under a very natural valuation, the set of identities of $\mathfrak{M}$ corresponds to the set of theorems of Jankov's logic, so that the theory of mass

problems provides a constructive interpretation of this logic.[3] However, Jankov's logic is structurally complete (*Prucnal 1976*). Thus one sees that the so-called weak law of excluded middle preserves the context insensitivity of CPC despite, standing in the place of full *lem*, allowing for a procedural semantics.

In my graduate student years, several of my friends and I were thinking about bounded arithmetic because of its connections with complexity theory and because the special difficulty of representing within these theories their own consistency statements shed much light on the fine details of arithmetization. We had a running gag, which is that formal theories like PA are awfully weak, because with them one can't draw very many distinctions. The implicit punchline, of course, is that the bulk of the distinctions one can draw in theories of bounded arithmetic are among statements that are in fact equivalent. Lacking the resources to spot these equivalences is no strength!

Something perfectly analogous happens in the case of substructural logics. There are theorems of CPC that are unprovable in IPC, but not vice versa, so the latter logic is strictly weaker. Moreover, CPC proves all sorts of implications and equivalences that IPC misses. But if we stop believing for a moment, as the discipline of logic demands we do, and ask about the fine structure of inter-dependencies among the formulas of propositional logic, IPC delivers vastly more information. Consider just propositional functions of a single variable p. In CPC there are exactly four equivalence classes of such formulas: those inter-derivable with p, $\neg p$, $p \vee \neg p$, and $p \wedge \neg p$. In IPC the equivalence classes of these same formulas exhibit a complicated pattern of implications, forming the infinite Rieger-Nishimura lattice.

One thought one may have is that IPC should be considered an expansion of CPC: Every classical tautology can be discovered with IPC via the negative translation of Gödel (*1933*) & Gentzen, so with IPC one gets all the classical tautologies and a whole lot more. (Gödel at times suggested something like this attitude.) But, of course, neither the negative translation nor the very idea of a classical tautology arises within intuitionistic logic. The thought that I encourage instead is this: The logician is loath to choose between classical and intuitionistic logic because the phenomena of greatest interest are the relationships between these logical systems. Who would have guessed that the rejection of a single logical principle would generate so much complexity—an r.e. set of admissible rules with no finite basis, an infinite lattice of inter-derivability classes?

The intuitionist and the classicist have very fine systems. Perhaps with them one gains some purchase on the norms of right reasoning or the modal structure of reality. The logician claims no such insight but observes that one can hold fixed the rules of the logical particles and, by merely tweaking the calculus between single conclusion and multi conclusion, watch structural completeness come in and out of view. The same switch, he or she knows, dresses the logical connectives up in a constructive, context-sensitive interpretation in one position and divests them of this interpretation in the other. These connections between sequent calculus, constructive proof transformation, structural completeness, and *lem* are fixtures from our logical knowledge store, but they cannot seriously be thought of as a network of consequences in some allegedly correct logic.

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[3]For the details of this interpretation, see *Terwijn 2004*

3. Logic imposed and logic discovered

If I have conveyed my attitude successfully, then I will have inspired the following objection:

> You speak about an unwillingness to embrace any one or select few logical systems because of an interest in understanding all such systems and how their various properties relate to one another. But by making logical systems into objects of investigation, you inhabit an ambient space in which you conduct this investigation. It is legitimate to ask which logic is appropriate in this space. What is your 'metalogic'?"

I shall explain that this objection rests on various preconceptions that I do not share. I hope the explanation functions to aid the reader in seeing these as misconceptions. If it does, then logical nihilism will be understood.

If we agree that as logicians we are interested, not in factual truth, but in the relationships among phenomena and ideas, then the point of view we must hasten to adopt should be the one that assists us in detecting and understanding these relationships. Which relationships? Presumably, it has been suggested, those that accurately pick out grounds and consequences, those that answer the question "What rests on what?" But why stop here? What sort of purpose is served by simultaneously disregarding factual states of affairs and pledging allegiance to factual relations of ground and consequence? I have an intuitive sense of what a fact is; I have no such sense of ontological grounding. Nor have I seen any reason to expect that the study of logic can foster such a sense in me.

More appropriate seems to be a disregard for privileged relationships similar to our disregard for truth. Suppose that we are interested in detecting and understanding whatever relationships we can find. Then we might wish not to be wedded to any point of view. We might, instead, try on a few hats until some interesting patterns appear where before there seemed to have been only disorder. We might find that one hat helps time and again, but we will be well-advised not to forget that we are wearing it. For if we never take it off, then we risk forever overlooking logical relationships of considerable interest. Worse, we risk coming to think of the relationships we can detect as "in the world," "preconditions of thought," or some such thing.

Allow me to illustrate this point with an example. At least since Aristotle it was expected that great complexity could be uncovered by a proper analysis of the quantifiers that occur in natural language. Notably, quantifiers allow us to reason in succinct strokes about infinite collections. About a century ago, David Hilbert had the idea of analyzing quantifiers with consistency proofs, and he devised an intricate calculus with "transfinite axioms" which allows proof figures involving quantifiers to be transformed (in principle) into figures without them. Hilbert's idea was that reasoning about this transformation (which by its nature requires being attentive to constructibility) would expose the quantifiers as innocuous parts of our mathematical language and also make perspicuous their complexity.[4]

Hilbert conducted this investigation in the shadow of a great ideological quarrel about the validity of various logical and mathematical principles. In one quarter were dour skeptics who distrusted not

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[4]For details, see section 2 of *Franks 2014*.

only *lem* but other forms of infinitary reasoning. Chief among them was Kronecker, against whom Hilbert (in *1922*) railed because he "despised . . . everything that did not seem to him to be an integer." Less famously, but perhaps more importantly, he faulted Kronecker also because "it was far from his practice to think further" about what he *did* accept, "about the integer itself." In a similar vein, Hilbert observed that Poincáre "was from the start convinced of the impossibility of a proof of the axioms of arithmetic" because of his belief that mathematical "induction is a property of the mind" (201). Thus Hilbert viewed these figures as short-sighted, not only in their rejection of mathematical techniques that he wished to defend, but also in their belief that the manifest validity of a principle precludes any hope of our analyzing it. Both attitudes, he cautioned, "block the path to analysis" (199).

After only a decade of partial successes, it was discovered that the sort of consistency proofs Hilbert envisioned are not available. Specifically, Gödel (*1931*) demonstrated that no proof of the consistency of a reasonably strong and consistent mathematical system could be carried out within that same system. Typically it is the recursion needed to verify that the proof transformation algorithm halts that cannot be so represented. This situation raises the question whether proving with such principles that a system is consistent is not *obscurum per obscurius*.

In *1936* Gentzen made these circumstances much more precise by providing a perspicuous proof of the consistency of PA. Gentzen's proof is carried out in the relatively weak theory PRA together with the relatively strong principle of transfinite induction up to the ordinal

$$\epsilon_0 = \sup_{n<\infty} \omega_1^{\omega_2^{\cdot^{\cdot^{\cdot^{\omega_n}}}}}.$$

Together with Gödel's result, this proof demonstrates that transfinite induction through ϵ_0 is unprovable in PA. All of this is well known. Most people familiar with the history of logic are aware that Gentzen proved also that transfinite induction to any ordinal beneath ϵ_0, any ordinal $\omega_1^{\omega_2^{\cdot^{\cdot^{\cdot^{\omega_n}}}}}$ for $n \in \mathbb{N}$, is in fact provable in PA. But Gentzen pressed even further. One can consider fragments of PA defined by restricting the induction scheme to formulas with a maximum quantifier complexity (call these the theory's class of inductive formulas). Gentzen showed in *1943* that the height of the least ordinal sufficient for a proof of the consistency of such a fragment corresponds with the quantifier complexity of that theory's class of inductive formulas, and that transfinite induction to any smaller ordinal is provable in the fragment. So the number of quantifiers over which mathematical induction is permitted equals the number of exponentials needed to express the ordinal that measures the theory's consistency strength. One quantifier equals one exponential. Thus the theory of constructive proof transformations has turned up a precise mathematical analysis of the complexity of natural language quantifiers, a remarkable realization of Hilbert's original ambition.

Why has so little attention been given to this result? The discussion of Gentzen's work has been dominated by debate about whether or not the proof of PA's consistency can really shore up our confidence in this theory. To anyone who has witnessed a talk about ordinal analysis sabotaged by this debate, the scene will be familiar: Someone reminds us that the proof uses a principle that extends the resources of PA. Someone else defends the principle despite this fact and points out that in every other way the proof is extremely elementary compared to the full strength of PA. Because the two theories PA and PRA+ti_{ϵ_0} are in this basic way incomparable, the jury is out as to the the

gains made by reducing the consistency of one to that of the other. From the point of view of logic, however, this is all a distraction from what Gentzen actually achieved: He showed that the question of the consistency even of elementary theories can be formulated as a precise problem, and he showed that the solution to this problem requires new perspectives and techniques and carries with it unexpected insights about logical complexity. If philosophers did not harbor skepticism about PA, then they would likely not be interested in Gentzen's result one way or the other. Their disinterest in the analogous results about fragments of PA is just evidence that they harbor no skepticism about these theories. We recognize Gentzen's analysis of first-order quantifiers as one of the deepest results in the history of logic as soon as, and no sooner than, we stop believing.

I now wish to respond more directly to the objection that opened this section. Of course it is true that in the study of logical systems one must engage in reasoning of some sort or another. This reasoning can possibly be described by one or a few select logical systems. But why should anyone assume that this amount of reasoning is anything more than ways of thinking that have become habitual for us because of their proven utility? Further, why should anyone assume that there is any commonality among the principles of inference we deploy at this level over and above the fact that we do so deploy them?

To expand on the first of these points, it may be helpful to draw an analogy between rudimentary logic and set theory. Often it is thought that decisions about which principles should govern the mathematical theory of sets should be made by appealing to our intuitions about the set concept and even about the cumulative hierarchy of sets. Doubtless such appeals have figured centrally in the development of set theory. But the history of the subject suggests that a complete inversion of this dynamic has also been at play. Kanamori explains:

> [L]ike other fields of mathematics, [set theory's] vitality and progress have depended on a steadily growing core of mathematical proofs and methods, problems and results. …from the beginning set theory actually developed through a progression of *mathematical* moves, whatever and sometimes in spite of what has been claimed on its behalf. (*2012*, 1)

Whereas one can today find endless phenomenological and metaphysical justifications of, for example, the replacement axiom, Kanamori contends that set theory in fact evolved primarily by absorption of successful techniques, like transfinite induction, devised to answer mathematical questions. "It was von Neumann's formal incorporation of this method into set theory, as necessitated by his proofs, that brought in Replacement" (33). In similar fashion, the power set existence assumption, which originally had many detractors, was not finally embraced in the wake of any argument or philosophical insight. It merely happened that "iterated cardinal exponentiation figured prominently" in Kurepa's proofs in infinitary combinatorics, "so that shedding deeper concerns the power set operation became further domesticated …" (46) The upshot? "Set theory is a particular case of a field of mathematics in which seminal proofs and pivotal problems actually shaped the basic concepts and forged axiomatizations, these transmuting the very notion of set" (1).

The point is not that we have no reliable intuitions about the set concept, nor even that they should play no role in the development of formal set theory. The point is that those intuitions have

evolved in response to our need to make sense of routinely counter-intuitive scientific discoveries. If our intuitions have been at least partially, perhaps largely, shaped by developments in logic, then the fact that we appeal to them on occasion in order to refine our definitions and techniques or in order to choose new axioms seems to lose its significance. Rather than develop a theory of sets that unpacks the fundamental truths we intuit, we have developed an intuition about sets that makes sense of mathematically interesting relations we have discovered.

I need not argue that rudimentary logic has taken shape in a similar fashion first because Professor Maddy has been persuasive on this same point in her contribution to this volume and second because it does not really matter for my purposes whether, in the end, this is true. I am content simply to reveal the picture that holds us captive when we begin to think about logic. For the objection we solicited was that if our modern science of logic thrives on an unprejudiced consideration of the full gamut of properties exhibited by logical systems and the relations among them, so that issues of correctness do not arise, then it has only smuggled those issues in through the back door, in the metalogic that makes the science possible. But it is a preconception that science is made possible by ahistorical norms of right reasoning. Once one considers the possibility that logic may be studied with patterns of thought adapted to what we learn along the way, it becomes hard to understand what special status the rudimentary principles we find ourselves reflexively appealing to are supposed to have.

This brings us to the second of the points above, the idea that the principles that have found their way into our basic toolkit must presumably have some features in common that led them there. Even if these principles have been adopted over the course of time, the idea goes, there must be some reason for their being adopted instead of other principles. Perhaps this reason can be repackaged as an explanation of their being the true logical principles.

In response to this suggestion, I wish only to expose the presupposition driving it. Whoever said that there must be some property of logical validity that some principles of inference enjoy and others do not? If we knew in advance of all evidence that some such property attains, then it might be reasonable to look for it in whatever classes of inference rules we happen to find collected together. But we have no such foreknowledge and, in fact, the evidence suggests that the arrangement of our toolkit is a highly contingent matter. Are we not better off shedding this vestigial belief that among all the intricate and interesting consequence relations out there, some have a special normative status? Can we not get by with the understanding that principles of inference with a rather wide range of applicability differ from those suited only to specific inference tasks *only* in having a wider range of applicability? Had this been the understanding that our culture inherited, would anything we have learned from studying logic lead us to question it?

At the end of Plato's *Phaedrus*, Socrates explains that prior to investigating the essence of a thing, it is important to devise an extensionally adequate definition of that thing so that we will be in agreement about what we are investigating. This attitude seems right to me, and it seems to me that the familiar debates about, for example, where logic leaves off and mathematics begins violate this principle unabashedly. Suppose it were clear to everyone that some but not all patterns of reasoning are inescapable and furthermore that it were easy to tell which these are. Then we would have good reason to label this "logic" in distinction to patterns of reasoning that we all recognize as the province

of some special science or particular application. It would be reasonable to wonder what accounts for the privileged role that these principles play in our lives. As things stand, however, many of us seem instead to assume that there simply *must be* patterns of reasoning that differ in kind from others. Typically, our minds are already made up about the psychological or metaphysical circumstances that underwrite this difference. This is what Wittgenstein stressed with his observation that "the crystalline purity of logic was not the result of an investigation," that instead "it was a requirement" (§107). Driven by this assumption, we thrash about looking for some extensional definition that we can hang our ready-made distinction on. These definitions are simply unconvincing on their own. They can satisfy only people who cannot tolerate the thought that there is no line to be drawn.

When Gentzen began his study of logic, he parted ways with his predecessors[5] by not first defining logical validity and then seeking out logical principles that accord with that definition. He simply observed that some patterns of reasoning abound. Consider that to define "predicate logic" he said only that it "comprises the types of inference that are continually used in all parts of mathematics" (*1934–35*, 68). This homely definition set Gentzen on a task of empirically tallying the techniques used in mathematical proofs, ignoring those that are unique to geometry, arithmetic, and other specific branches of mathematics.[6] Of course such a survey is by no means guaranteed to be exhaustively executable, and it was Gentzen's good fortune that his subject matter happens to exhibit few instances.

One should not, however, write off his success as purely a matter of luck. Gentzen devised an ingenious argument to the effect that his tally was in fact exhaustive. This involved constructing an innovative type of logical calculus that is at once formal and patterned on the informal reasoning recorded in mathematical proofs. This scheme enabled him to do more than construct a serial tally of proof techniques, because the inference types identifiable with it are extremely few and systematically arranged so that one can be sure that none have been overlooked. After all, if there are any inference types that went unnoticed, then for that very reason they fail to meet the criterion of ubiquity in mathematical practice that Gentzen imposed.

This empirical "completeness proof" bears little semblance to familiar conceptions of logical completeness and is interesting for this reason. I mention it now only to draw attention to the fact that while Gentzen's definition of predicate logic does pick out a well-defined body of inferences,[7] he did not concoct the definition in the service of a preconceived notion of logical validity. He did not, for example, first stipulate a semantic notion of logical consequence based on his own intuitions and then ask whether his calculi adequately capture this notion. Gentzen simply proposed that the intuitions guiding mathematicians in their research would be worth isolating and studying, and he therefore modeled a logical calculus on the inferences mathematicians actually make.[8]

Of course mathematicians also deploy proof techniques that are less universal, and the only observable difference between these and the ones that meet Gentzen's criterion of ubiquity is their relative infrequency. Mathematicians do not report a feeling that arithmetical reasoning is less valid

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[5]There are historical precedents for Gentzen's attitude: Aristotle, Condillac, Mill.

[6]This is explicit in *Getnzen 1934-35* and even more vividly depicted in section 4 of *Gentzen 1936*.

[7]Actually Gentzen vacillated over the inclusion of the principle of mathematical induction, ultimately deciding against it.

[8]For details of this conception of completeness, see section 3 of *Franks 2011*.

or valid in some other way than general reasoning, and even if they did, we should be inclined to ignore these reports if they did not reflect in mathematical practice. For these reasons Gentzen could never bring himself to describe the distinction between inference rules that appear in the predicate calculus and those that belong specifically to arithmetic as a distinction between the logical and the non-logical. He only thought that he had designated *a* logical system, one that by design encodes some of the inferences he was bound to make when reasoning about it but whose logical interest derives solely from what that reasoning brings to light.

Contrast this with one of the more famous attempts to demarcate the logical: Quine's defense of first-order quantification theory. Second-order quantification, branching quantifiers, higher set-theory, and such can each be dissociated from logic for failing to have sound and complete proof systems, for violating the compactness and basic cardinality theorems, and other niceties. There is even a theorem, do to Lindström, to the effect that any logic stronger than first-order quantification theory will fail to exhibit either compactness or the downward Löwenheim-Skolem theorem. Second-order logic, Quine concluded, is just mathematics "in sheep's clothing" because by using second order quantifiers one is already committed to non-trivial cardinality claims (*Quine 1986*, 66).

How true will these remarks ring to someone who doesn't know in advance that they are expected to distinguish logical and mathematical reasoning? Quine's consolation is telling: "We can still condone the more extravagant reaches of set theory," he writes, "as a study merely of logical relations among hypotheses" (*Quine 1991*, 243). I should have thought that this accolade, especially in light of the intricate sorts of logical relations that set-theoretical principles bear to one another and that set theory bears to other systems of hypotheses, would be used rather to enshrine a discipline squarely within the province of logic. For if we never suspected that among the plenitude of logical relations are a privileged few that capture the true inter-dependencies of propositions, what else would we mean by "logic" than just the sort of study Quine described?

As to the properties that characterize first-order quantification theory, it should now go without saying that from our perspective Lindström's theorem, far from declaring certain formal investigations extra-logical, *exemplifies* logic. So too do results of Henkin (*1949*) and others to the effect that second-order quantification theory and first-order axiomatic set theory each are complete with respect to validity over non-standard models. For a final example, I can think of none better than the recent result of Fan Yang that Väänänen's system of dependence logic (with branching first-order quantifiers), extended with intuitionistic implication (the context sensitive and constructive operator that results from the denial of *lem*), is equivalent to full second-order quantification theory (*Yang 2013*).

Wittgenstein (§108) expected his readers to recoil from the suggestion that we shed our preconceived ideas by "turning our whole examination around." Rather than impose our intuitions about logic on our investigations by asking which principles are truly logical, let us first ask if a close look at the various inference principles we are familiar with suggests that some stand apart from others. If some do, then let us determine what it is that sets them apart. But the question he puts in our mouths—"But in that case doesn't logic altogether disappear?"—suggests that we know deep down that our empirical investigation is bound to come up empty. Various criteria will allow us to demarcate different systems of inference rules to study, but when none of these indicate more than

a formal or happenstance association we will find ourselves hard pressed to explain why any one of them demarcates "the logical."

I am more optimistic than Wittgenstein. The conclusion that I expect my reader to draw from the absence of any clear demarcation of the logical is not that there is no such thing as logic. Let us agree instead that no one part of what logicians study, contingent and evolving as this subject matter is, should be idolized at the expense of everything else. Logic outsrips our preconceptions both in its range and in its depth.

4. Conclusion

Traditional debates about the scope and nature of logic do not do justice to the details of its maturation. In asking whether certain inferential practices are properly logical or more aptly viewed as part of the special sciences, for example, we ignore how modern logic has been shaped by developments in extra-logical culture. Similarly, questions about whether logic principally traces the structure of discursive thought or the structure of an impersonal world presuppose a logical subject matter unaffected by shifts in human interest and knowledge.

I mean, by saying this, not just to suggest that the principles of rudimentary logic are contingent, not different in kind from principles that we use only some of the time or very rarely and only for specific tasks. I do urge this attitude. But the caution against mistaking our default, multi-purpose habits of reasoning for something monumental is only preparation for a second, more valuable reaction. One should warm up to the trend of identifying logic with the specialized scientific study of the relationships among various systems and their properties. This is, after all, how logicians use the word. Our preference to ignore questions about a logic's correctness stems not only from an interest in exploring the properties of possible logical systems in full generality but also from an appreciation, fostered by the study of logic, that no one such system can have all the properties that might be useful and interesting.

In closing, let me re-emphasize that the idea of a true logic, one that traces the actual interdependencies among propositions, is unscathed by all I have said. Part of the difficulty in questioning that idea is that it is a moving target: Argue against it, you feel it again in that very argument; Close the door, it will try the window. But this very circumstance only underlines the fact that the idea is a presupposition, nothing that emerges from any discovery made in the study of logic. For the same reason that we can marshal no evidence against it, we see that if we can manage to forget it our future discoveries will not reveal to us that we have erred.

This realization, coupled with the observation that a fixation on the true logical relationships "out there" hinders the advancement of logic, certainly recommends nihilism on practical grounds. The question that remains is whether we are capable of sustaining a point of view with no direct argumentative support.

The proper antidote to our reflexive tendencies will surely extend an analysis of modern logic and include a rehearsal of the subject's history. I cannot offer that here.[9] I can only mention that

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[9]For the details of the evolution of one central concept—that of logical completeness—in the past two centuries, see *Franks 2013*.

logic as a discipline has evolved often in defiance of preconceived notions of what the true logical relations are. Logic has been repeatedly reconceived, not as a fallout from our better acquaintance with its allegedly eternal nature, but in response to the changing social space in which we reason. There is reason neither to expect nor to hope that logic will not be continually reconceived. Such reconceptions have been and likely will again be fundamental, so that what makes the moniker "logic" apt across these diverse conceptions is not an invariable essence.

In these pages I have indicated instead logic's modern contours, highlighting the fact that the deepest observations logic has to offer come with no ties to preconceptions about its essence. The richness of logic comes into view only when we stop looking for such an essence and focus instead on the accumulation of applications and conceptual changes that have made current logical investigations possible. The study of logic might be the best practical antidote to the view of it that we have inherited.

In his *Logic* of 1780, Condillac wrote: "People would like to have had philosophers presiding over the formation of languages, believing that languages would have been better made. It would, then, have required other philosophers than the ones we know." Our interest in a better made language is an interest in a language that traces a pre-existing logical structure. Like Condillac, Wittgenstein warned that presupposing such a structure fosters dismissive attitudes about the languages we have: "When we believe that we must find that order, must find the ideal, in our actual language, we become dissatisfied with what are ordinarily called "propositions," "words," "signs" (§105). When we stop believing for a moment, as the discipline of logic demands we do, the structures we find immanent in our several, actual languages command our interest more than anything we could have devised in the service of our ideal.

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